the formative laws governing the organism a dozen or more layers of cells surrounding the embryo of the wheat or maize are completely absorbed and in the end the innermost remaining walls of the ovary are literally cemented to the outer unabsorbed layer of the inner integument.[17] Here is emphatic control of cell walls by the life inhabiting them, control exerted chiefly through the agency of the Ca-ion-equilibria of the tissues concerned. Finally this control in the wheat as in Herbst's sea urchin embryos is shown by the fusing together of *outer surfaces of cell walls*. Here we seem to have clean cut instances to show how in the formative processes the living material is able to command the structure it forms about itself. The outer walls of cells originally located far from each other are brought together by the solution of intervening structures. The substances necessary for the formation of the cementing layer seem to be extruded from the protoplasm through the wall to the outside surfaces where they unite to form the coagulum seen. Perhaps the Ca ions and the pectase thrust through from the interior of the cell meet at its frontier the pectin which under enzyme action yields pectic acid in the presence of the Ca ions. The product of such an occurrence would be seen in the cementing layer formed on the outside of each of the now neighboring cells.

In conclusion, I should like to refer briefly to some of the more practical results that seem to flow from the considerations that have been here set forth.

It appears that a certain quantity of Ca ions must be present in the medium for the maintenance of the chemical and functional integrity of the cell wall, as well as the chemical and functional integrity of the deeper lying living parts of the cells of absorbing roots of higher green plants. When this is so maintained, absorption takes place in the manner we are accustomed to call normal. When this necessary minimal supply of Ca ions in the medium is lacking, be it in soil solution, water culture, or sand culture, the function of absorption is upset and a more or less marked leaching of ions from the plant follows. In the absence of this necessary minimum of Ca ions, the soil solution or culture solution may be rich in all other required ions, but these are useless to the plant. They are unabsorbable. This brings us face to face with a condition of affairs in plant nutrition that has not been recognized and therefore has not been characterized. We may fairly say that Ca ions make *physiologically available* other equally indispensable nutrient ions. The practical consequences that follow from this way of looking at the fertilizer problem have not thus far been realized. We learn why from earliest times civilizations have grown up on soils rich in limestone dèbris. We learn why agriculture has readily succeeded in some regions, not in others. We understand why, by the use of lime, lands have been rendered capable of supporting largely increased populations. We are now able to correlate these broad facts with those of cell physiology and to suggest perhaps not *the calcium function* sought by Jost, but one way perhaps of many in which higher green plants find calcium necessary.

17 True, R. H., *Bot Gaz.*, 18: 212-226. 1983.

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THE METHOD OF SCIENCE IN AGRICULTURE[1]

To be practical has been the great goal of agricultural investigation from the beginning. It was entered upon with a practical purpose, and in a large degree practical results early came to the expectation of the farming people. Here was a type of science which was not working in the clouds for its own sake, but down in the dirt where the problems of farming lay.

It is fortunate that this has been so—that this close sympathy and this urge to meet the needs of the art have been felt so keenly. It has given life as well as purpose to our branch of science, and the wide extent to which its

1 Address of the Vice-president and Chairman of Section O—Agriculture, American Association for the Advancement of Science, Toronto, 1921.

findings have been embraced and woven into the warp and woof of intelligent practice has been a constant source of stimulation. It makes even more imperative the call for steady progress, not only in getting practical results for immediate use, but in securing deeper insight and larger intelligence about the common things of agriculture.

The problems of agricultural science have become increasingly difficult. As the simpler things lying near the surface are gradually solved the underlying problems are seen to be more complex and difficult, taxing knowledge, skill, and imagination to increasing extent. Almost have they come to call for that rare perspicacity of the colored preacher who claimed to be able "to explain the unexplainable, to make known the unknowable, and to unscrew the inscrutable."

At all events, there is no more exacting field of experimental inquiry at the present time, and success in it is largely a matter of methods. It calls for a clear conception of the nature of problems and means for deriving the needed data for their solution. Steady advancement in some of the oldest and most common lines of agricultural inquiry rests more largely on the development of methods than on additional experiments or the accumulation of data on the conventional basis. It is the largest problem in agricultural investigation at the present time, and it is so important that in a large degree it determines the progress of science.

Fundamentally the method of science is the same, of course, in agriculture as in the simple sciences. It makes no difference whether the subject is cornmeal or a chemical compound, the response of the growing plant or the law of falling bodies, the experimental method and requirements for the same grade of inquiry are the same. But in practice different types of effort are represented which vary with respect to their aim and the extent to which they require application of the scientific method. The difference is perhaps chiefly a quantitative one, of degree rather than kind, in conception of the end of inquiry rather than in general essentials which must be met.

In the simpler form of agricultural work, consisting of observations, tests and trials, the object may be a quite superficial one—the attempt merely to get a bit of information but one step removed from ordinary experience, such as the profit from use of a fertilizer, the larger crop from spraying, or the advantage of fall plowing. The information may be quite sufficient for the practical purposes of the time and place, but it can not be said to be very scientific, even if made with every care, for the work involves no study of exact relationships or tracing of the effect of conditions. In other cases observations, tests and trials may have a deeper purpose and form a step in investigation. Similarly, experiments may be purely comparative, as showing the relative value of different fertilizers, or feeding stuffs or methods of tillage, without touching any basic fact; or they may be the means of securing scientific facts in a piece of fundamental research.

In the early stages of agricultural experimentation, before the problems had been organized to show their nature and content, the work was naturally elementary, based largely on observations, comparative trials, and simple experiments which did not attempt to determine the underlying conditions or establish definite relationships. These types of work have given results which although largely empirical have been extremely useful. They have supplied a great fund of information on which to develop practical systems and to base further experimental inquiry. Although sufficient for one stage, they may be a poor means of progress in another. Hence they need to be replaced by more rigorous methods and by investigation which goes to the heart of the problems.

It has been a somewhat prevalent mistake to assume that a complex agricultural problem could be solved in its practical aspects without a study of the principles and factors underlying it. This has led to the attempt to secure quick results by short cuts, and has bred overconfidence in the competence of simple comparative experiments. Reliance upon such time-honored procedure in certain classes of work has re-

sulted in the effort to refine them without going outside of them or bringing to their support more abstract types of inquiry which the changing status of the problems made necessary.

This is not to overlook or to minimize in the least the increasing extent to which agricultural research has advanced into new fields or stages of inquiry, has developed improved methods and means of progress, and has been rewarded with results comparable with those in any line of investigation. Such effort has well illustrated the truth that in this branch of research as in other walks of life "we build the ladder by which we rise"; and it argues for a type of experimental work which is critical of its methods and conclusions, seeking means for strengthening them and avoiding error or uncertainty. But certain types of work have not been marked by such growth of vision and method, with the result that they have become doubtful means of scientific progress at the present time. They continue to perpetuate their possible errors or inherent limitations after these have been disclosed. They are not fulfilling the expectations originally placed upon them; and while they have been useful up to a certain point, they are accumulating data after they have ceased to shed new light.

The aim of science is simplicity, the dissolution of complexities, and development of simple facts and statements easily comprehended. Its method begins with a simplifying process, the analysis of problems to get at their real nature and content, the resolution of complex questions into parts which are sufficiently simple and self-contained to be capable of study. Often this can be only partially done at the outset, but as the investigation proceeds and the real nature of the problem is disclosed, the segregating process becomes easier.

In agricultural investigation this is difficult because of the many factors embraced, and in the more common types of work with plants and animals it has been followed to only a limited extent. More often the problem has been an involved and complex one from the start, embracing a wide range of phenomena, and instead of being simplified and reduced to smaller definite units as the work progressed, it has gathered bulk as it went, like a snow ball, until it has become such a complicated aggregation as to be well-nigh unworkable. Too large for any intimate study, the mechanics and routine of it have occupied the full time, and it has often degenerated into the broad accumulation of data.

In constructive research data are secured for use, not for themselves. They are designed for a definite purpose—to solve a concrete problem, to prove or disprove a conception or an idea, to disclose scientific facts. The undirected collection of facts, whether they be observations, results of experiments, or what not, leads to complexity, to an aggregation of data which must first be classified before being used in molding a scientific explanation or a principle, or developing even practical information. Unless there is a clear objective and an idea to guide in the acquiring of data, it may be a waste of time, an aimless, hopeless, dead effort. Its results may be chaotic, impossible of developing a leading principle or an illuminating fact.

There is still a quite prevalent idea that the ends of research may be satisfied by the accumulation of data. It is a common expression in connection with the status of long-continued experiments that data are being accumulated. This is especially apt to be the case where such complex conditions and factors are involved that the results from year to year are confusing, and it is assumed that these uncontrolled variables may be eliminated by long repetition. In such cases there is apt to be lack of a critical attitude toward both the method and the data themselves, and hence the test of adequacy or competence is not applied. Data add to the accumulated fund of information when they are accurate, systematic and orderly, and so capable of enabling deductions or fitting into other supplies which may be so used. Unless they respond to such a test it may well be questioned whether their accumulation is profitable at this stage, when there is already such a large background.

Simplification and definiteness of purpose give direction to the making of records and the gathering of data. All experimental inquiry turns upon securing proof which is both accurate and adequate to the purpose. The method of science is the process of securing accuracy and precision in purposeful observation, and the interpretation of the product. As has been said, it is "only a perfected application of our human resources of observation and reflection."

The method is not a fixed thing but is continually changing as progress makes possible. Science strives constantly after new ways of acquiring and proving facts which would otherwise not be known or but imperfectly so, and at the same time eliminating the personal factor. Apparatus and appliances are designed primarily to make possible the taking of observations which would otherwise not be feasible, or with equal accuracy. They therefore enlarge the field of observation and increase precision.

This applies of course to facilities and methods for agricultural inquiry such as field plats and cylinders, feeding appliances, special apparatus and other means for securing experimental data; and there is the same need of critical examination of these from time to time that there is of other facilities, to determine whether they are supplying proof which is accurate and sufficient, or to assess correctly what can and what can not be shown by such methods.

The question is forcing itself upon the minds of many as to the adequacy of certain types of field experiments, as ordinarily conducted, to answer fundamental questions in plant nutrition and soil management. Large reliance has been placed on such experiments in the past, and data have been accumulated from them over long periods. The oldest series of fertilizer and rotation plats in this country runs back over forty years; several others have been under way from twenty-five to thirty-five years. One station has some two thousand plats.

These experiments have brought highly important practical results, and have marked a definite step in agricultural inquiry. They have furnished a rich background of material and suggestion for more definitely directed studies. The question is whether they have reached their maximum and how far they are to be depended upon in making further advances.

It is now realized that many of these experiments contain inherent difficulties dating back to their beginning, which introduce a strong element of doubt in interpreting results. For one thing, most of the published reports fail to describe the soil except in the most general way, and lack information as to the condition and previous treatment of the field, indications of irregularity, etc. Again, the number of check plats is usually too small, and the same is true of the amount of replication of treatment. This may account for the different interpretations made by different persons from the same series of experiments. In few cases has the necessary number of checks and duplicates been worked out mathematically for such experiments, and where there is considerable variation in different parts of a field, averages may furnish a doubtful basis for measuring the effect of treatments.

The number of questions "put to the soil and the plant" in a given plat experiment has usually been far too large. For example, the customary rotation-fertilizer experiment has often covered practically the whole range of soil fertility and plant nutrition. This wide range has limited the amount of replication practicable, and it has failed to reflect the discrimination in gathering data and the simplification of the problem dictated by the method of science.

Such experiments have relied quite largely on what the field results themselves were interpreted to show, primarily the crop returns. True, most of the later experiments have embodied plans for chemical, bacteriological, and other laboratory studies, but only to a limited extent have these been developed with the progress of the work so as to shed new light. The chemical studies have often become of a routine nature—analyses of the crops and of the soils at stated intervals, and the bacteriological studies by the technique developed

have largely failed to meet expectations in establishing correlations between soil treatment and bacterial flora. Such bacteriological observations have now almost ceased in connection with long continued field experiments.

Reduced to such a simple collection of experimental data, the conduct of these extensive field experiments has often become largely a matter of routine. The niceties of plat work are observed, but the element of actual inquiry is deferred until many years have supplied their data. When that time is reached the publication is more often a summary of field and and laboratory records than a critical analysis of the data and their actual meaning. At best the product is quite apt to consist of empirical observations rather than definite contributions to fundamental principles. We have not yet learned how to interpret, except superficially, the answer which the soil and the plant give as to just what has happened or what the apparent effects are due to. We have not yet learned how to examine a plot of soil so as to determine the changes occurring from time to time or brought about by a long continued system of treatment, or how to connect these changes with the response of the crop in a given season or period. Indeed, relatively little study is now given in such experiments to the soil itself, and only to a limited extent are underlying questions suggested by such experiments being given intensive study.

In a word, the indications are that in the majority of cases the use is not being made of such long-time field experiments that ought to be made at this stage. They are rarely being simplified as time goes on, with a narrowing down to specific problems for intensive research, and they are not being increasingly supplemented by definitely directed laboratory study. They ought themselves to be progressive both in method and outlook. They ought to be used as the source of problems and material with which to make further and more profound inquiries.

We can hardly fail to recognize the changed status at the present time, both as to practical requirements and the stage which has been reached in research and its problems. What is especially needed at this stage is the study of factors and their relationships rather than gross comparisons of one complex of conditions with other complexes. This will call for the kind of team work which has been applied to the Rothamsted experiments,—the association of the chemist and the bacteriologist with the agronomist and soil expert, and the guidance of the statistician in both planning and interpretation.

In many of the feeding experiments, also, the unchecked sources of possible error are too great for safety. The small number of animals in the lots gives large chances for the influence of individual variation. The conditions and frequency of weighing may also give misleading indications. Some of the results of such experiments can be measured quite accurately, while others can only be described. Some are not strictly experimental because they embody so many factors not under experimental control and whose probable variation can not be estimated. This is true, as Dr. H. H. Mitchell has recently shown, of the cost or financial returns in feeding. Such results lack permanent value, and are likely to be given a prominence and an application which they are not entitled to.

Experiments of this practical type have been useful in the past and there will be need for them in future. It is important that they occupy their proper place; but in the scheme for investigation they should not take the place of nutrition studies based on more permanent factors than prices and food combinations, or reliance rest too largely on them at this stage.

Many important advancements have been made in animal nutrition which will find application in feeding practice and in showing the reason back of it. These disclose more clearly the functions to be discharged by food, the inherent qualities which account for the observed value or special properties of feeds, and the means of measuring the response of the animal with a high degree of accuracy. Such fundamental investigations ought assuredly to be encouraged, not to the exclusion of but along with the type of feeding experiments which seek a more immediately practical end.

There is still need to cultivate intelligent public appreciation of research conducted in accordance with the spirit and the method of

science. It has been far easier to get funds for types of work which promise early contributions to practice than those which dig deep and lay solid foundations to make the whole superstructure sure. The dependence of the former upon the latter needs to be recognized.

The magnificent work of Armsby and his associates has been the admiration of the scientific world, but in spite of its ultimate practical value, and especially in furthering investigation, it had not within itself the elements of publicity, and was only vaguely understood. It never had an assured permanent income, and in that sense was obliged to live from hand to mouth. The loss this entailed is realized too late; and now the future of the work he so admirably started is under discussion. It would be a calamity if it were allowed to fall to the ground.

The large amount of attention now being given to fundamental and searching inquiry on the soil, the conditions of plant growth, and related subjects, should not fail of mention in this connection, for it illustrates the development of insight into these problems. At no period has there been anything comparable to it. The results which are following from these intensive studies amply justify the expectations of them as constructive means of progress.

With all the facts clearly in mind, it is very important to take an account of stock in the more conventional lines of experiment; to study seriously the long list of the better experiments in order to determine what they have actually shown, what they are competent to show, and the lessons they teach in methods. By all means, let us garner in all the teachings of these field and other common types of experiment; let us profit by both the good and the bad experience, but let not the negative results be overlooked in searching for the more positive ones. Such experiments represent large annual expenditures, and they occupy the time of a large body of workers. They express a confidence on which men are staking their efforts and their prospects. It is important to know the place which such experiments should occupy in future study and the manner in which they need to be supplemented. This may be one of the fundamental lessons to be drawn from them, and may indicate that their most useful field is in supplementing laboratory studies, rather than the reverse as at present.

In a public supported enterprise like agricultural investigation there must necessarily be a happy combination of effort representing different grades of intensity. Some problems or stages of them call more urgently for the full measure of the method of science than others, and it will be for the investigator to govern himself accordingly. But he can not fail to exercise a critical attitude toward all his work and his methods, or to exemplify in them the element of real progress.

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THE CONCILIUM BIBLIOGRAPHICUM

IN the issue of *Science* of December 2, I called attention to the critical situation in which I found the Concilium Bibliographicum this summer, when I made a special trip to Zurich to investigate this situation for the National Research Council and the Rockefeller Foundation.

On the occasion of this visit I proposed, after conferences with Mrs. Field (widow of the late Dr. H. H. Field), her business advisers, the chief of the technical staff of the Concilium, and official representatives of the Swiss Natural Science Association, which becomes under Dr. Field's will the legatee, under certain conditions, of Dr. Field's financial interests in the Concilium, a plan for an immediate temporary reorganization of the Concilium to last until January 1, 1922, and a further plan for a provisional permanent reorganization to go into effect as from that date.

The plan for temporary reorganization was put into effect immediately with Professor J. Strohl, of the Zoological Institute of the University of Zurich, as acting director, without salary. The proposed provisional permanent reorganization—by "provisional permanent" I mean a well considered and fully supported organization to run on until international mat-